DISCUSSIONS.

A NEW CLASSIFICATION OF THE SCIENCES.[1]

When a man has studied a problem for over twenty-five years, and then submits the results of his researches to the thinking world, his opinions certainly deserve more than a passing mention or a hasty review. This is the case of Prof. Adrien Naville, the Dean of the Philosophical Department of the University of Geneva, Switzerland. Lately he has condensed, within less than 200 pages, his ideas on the classification of the sciences,—a problem he has never lost sight of entirely, even when working in other lines.

The question, indeed, has grown in importance lately, owing to the new and more intimate relations which have been established between philosophy and science. After the grand, but illusory, speculations of the German Idealists in the first part of the nineteenth century, it looked as if philosophy was to fall forever a prey to metaphysics. And as this was also the period of the sudden and victorious efforts of the so-called positivistic sciences, which formed the honor of the last century, the rivalry only became more acute, and to many the rupture seemed definitive and irremediable.

But it was a false alarm. The old Aristotelian method combining observation and study of the real world on the one hand and speculation on the other, was bound to come to the top again. The last echoes of a long hostility proved more noisy than dangerous; and if they produced some sensation among people interested in these discussions, no true and conscientious scholar has allowed himself to be disturbed.

Thus everybody agrees that science and philosophy must proceed hand in hand; which does not mean, however, that their respective positions have as yet been clearly defined. Professor Naville's book reflects this situation very faithfully, indeed: reconciliation between the two on the one hand, but uncertainty concerning the method of collaboration on the other. Mr. Naville himself admits, with a frankness rare in a writer, that many a question remains open, and that

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[1] *Nouvelle classification des sciences. Étude philosophique.* Par A. Naville. Second, completely revised edition. Paris. Felix Alcan. 1901.

several merely provisional solutions will be found in the new scheme of the work which he proposes to the combined efforts of the laborers in all the different fields.

He first distinguishes between two possible principles of classifying the sciences: the objective principle which divides them according to their objects of investigation (botany which studies plants, astronomy which deals with stars, psychology which investigates the problems of the soul, etc.), and the subjective principle which classifies sciences by taking as a starting-point the specific way in which the subject reacts upon reality. The best known of these classifications is that of Bacon later also taken up with a few modifications by the French Encyclopædists of the eighteenth century: Man can approach the phenomenal world by three faculties of his mind,—memory, reason, and imagination; hence three great groups of sciences: history, philosophy, poetry,—and their subdivisions so entirely obsolete now.

The author of the *nouvelle classification* is in favor of the subjective principle; but giving up the old psychology of the faculties of the mind, he takes man as a whole, and the scientific attitude the latter can assume in face of the world is according to him a threefold one. He can ask: What is possible? What is it that actually is? What ought to be? From these three questions proceed three groups of sciences: those of the laws which govern the universe (*la théorématique*), those of the facts (*l'histoire*), and those of the ideal rules of action (*la canonique ou la poiétique*).

In his very sharp distinction between "laws" and "facts," Mr. Naville, it seems to us, has anticipated the spirit of the philosophy of to-morrow. To study laws—which in our modern system of thought correspond to the "types" or "ideas" of Platonism—was, in the past, one and the same thing as to study facts. To know the former was to know the latter also. It is hardly necessary to recall to mind such metaphysical systems as those of Spinoza,[1] Fichte, Schelling, and Hegel. To-day our philosophy is physical, that is, is based on our empirical knowledge of nature. In other words, laws are deduced from natural facts, and not natural facts from laws; or again, the facts are not in the laws, but the laws in the facts. This principle is not new. Not to go back to Aristotle and the Arabians, to Bacon and the Encyclopædists, Comte had taken it up and for the first time tried to apply it systematically through the whole range of human knowledge in his imposing *Cours de philosophie positive*. The progress of science has only confirmed its value. But the danger of an excessive reaction against the classical or metaphysical point of view has not been avoided by the majority of modern scientists. Just as it has been attempted to reduce the facts to a mere

[1] It is just, however, to recognise that a metaphysical system, in the strict sense of the word, is impossible and has never been conceived; a minimum of experience must always be allowed, even to *a priori* constructions like Platonism or Spinozism. See on this point our article in the *Revue philosophique*, January, 1899.

expression of *a priori* laws, an attempt has been made under the pressure of an altogether badly understood positivism, to reduce laws to facts. Not very long ago, a very strong inclination on the part of several scholars was noticeable, to doubt the validity of the very notion of law. Such extreme views could not in any event last forever, but a vigorous protest was necessary, and Mr. Naville will have been among the first to enter upon a new and undoubtedly more rational path, in according, so far as science is concerned, an independent and equal value to facts and laws.

We notice, however, that the *Théorématique* has been placed before the *Histoire*. Now, at the risk of being ourself considered a *réactionnaire*, and too much inspired still by the spirit of the science of yesterday, we feel inclined to think that Professor Naville has made undue concessions to the older metaphysical tradition If we wish to remain true to the modern point of view—which is indeed that of the *Nouvelle Classification*,—facts must come before laws: we deduce the laws from the facts; thus the historical sciences (by which the author means all the concrete sciences, such as astronomy, geology, physical geography, as well as human history in its different branches) must precede the theoretical sciences. It is very true that frequently we re-explain certain facts of history by the very laws which have been obtained from history; for instance, the laws of development of a given form of government which we have discovered while studying the history of one country, are applied by us afterward to the explanation of phenomena of the same nature in other countries. But according to Professor Naville, the true importance of both historical and theoretical sciences lies not in this reciprocal action,—though a great many scholars seem to delight in this rather shallow and aimless kind of work,—but in their practical application in the preparation of the world of the future. In other terms, they owe their importance to the existence of a third group of sciences,—that of the ideal rules of action. (See pp. 18–19, 145 et seq., "La question poiétique.")

One may be surprised at our criticism. The order followed by the author,—viz., What is possible? What is it that is? What is it that ought to be? seems at first sight very logical. It is so as long as one does not distinguish—but Mr. Naville does it very carefully elsewhere—between the metaphysical and the physical conception of phenomena, or better between the absolute and the relative standpoint. Absolutely speaking, the laws which govern phenomena must of course preexist. But the absolute standpoint is precisely not that of science; it cannot be; indeed, if science has gained such great victories in the practical world, it has only been by giving up all kinds of pretensions to the absolute. If, in itself, the logical order is that which goes from the cause to the effect, then, conversely, from the relative point of view of human knowledge, the logical order is that which goes from the effect to the cause. In other words, the laws are indeed prior to the facts, but the knowledge of the laws is after the knowledge of the facts; it is the history of human societies which has revealed to us the existence of sociology, and not the

contrary; it is the study of botany which has enabled us to formulate the laws of the growth of plants; it is the observation and description of stones which has provided us with the basis of mineralogy, and so forth; none of these groups of laws as known to us is *a priori*. Everywhere science has proceeded from history to theory.

One reason—besides that mentioned above—which may have induced Mr. Naville to adopt the order criticised by us, is that he has not deemed it necessary to separate his *Nomologie* from his *Théorématique*, but has put both together in a chapter following the *Histoire*. The advisability of this procedure is at least open to discussion. It seems that the fit place for the *Nomologie* was before and that of the *Théorématique* after the *Histoire*. The *Nomologie* answers the question, "What is it to know?" In fact the answer is presupposed by the term used (*Nomologie* = the science of the concept of law), and the phrase, theory of knowledge, which does not imply anything beforehand, would be preferable. We agree, however, in this, that we are looking for an order that prevails in the universe, and that we will have a principle of order if we succeed in discovering constant relations between phenomena; in other terms, if we reach the conclusion that the concept of law is the principle looked for. Now what one ought to understand by the word law, the elements which constitute the concept, the absolute or relative value of its revelations, its subjective or objective character, and so forth, that is the field of *Nomologie*. Thus the latter is not a science, as Professor Naville seems to suggest, but an introduction to science. As such it ought to be made to precede any study of reality, historical as well as theoretical, since it shows in what spirit we can approach this study. Not so with the *Théorématique* which has nothing to do with the general notion of law, but only deals with concrete laws, astronomical, psychological, social, etc.,—laws which, we repeat, we do not get at by pure reason, but through historical knowledge; laws which are only within our reach after the study of facts.

If we were to examine the subdivisions of both groups of sciences, *Histoire* and *Théorématique*, as proposed by Professor Naville, we should find them a new argument in favor of our point of view. As far as the historical sciences are concerned, the subdivision is in itself of no importance whatsoever. One thing only is certain, namely, that it is impossible for one scholar to embrace the study of universal history. It will therefore be necessary to adopt the method of a division of labor. But as science is not yet, at this stage of the process, in possession of a principle for systematic classification, it will be forced to take as a basis the objective appearance: the one will investigate the history of the stars, and the other the history of the earth, of plants, of the different branches of human history. This is purely conventional, and so it will continue until the scholar finds himself prepared to pass on to the *Théorématique*; for then we shall know which phenomena obey which laws; the historical facts will reveal that, but it is necessary to wait until they have taught us. That they do modify very often the data of com-

mon sense, no scholar can doubt to-day. Professor Naville writes in the very first pages of his book: "The distinction between physics and psychology, for example, remains profound; but it is, however, only a secondary distinction, not as profound as that which exists between psychology and political history, or between psychology and ethics."

We must now say a few words with regard to the third group of sciences proposed by our author, the group of the "sciences poiétiques" or of the ideal rules of action. From the very start we have to face the big problem of determinism or contingency in the laws of nature. It is easy to see the importance of it here: Suppose the existence of contingency (or liberty) duly established, a new factor will have to be taken into consideration in the science of the phenomena of the future, a factor not met with in the *Sciences théorématiques*; the *Canonique* would not be a simple prolongation or application of the latter. If, on the contrary, contingency is found to be no factor in the evolution of phenomena, a new group of sciences becomes all at once useless; for in this so-called group, what could be done except draw the conclusions from the *Théorématique*: given such and such facts and given such and such laws, such phenomena will take place? It would be a mere repetition, only with regard to the future, of what has just been found in the theoretical sciences.

A strong reaction has for some years been noticeable in favor of moral liberty as against determinism. It is manifest more or less in all domains of human activity and thought, in literature, in philosophy, in several forms of mystic theologies, even in physics. The book of a countryman of Professor Naville, the eminent physicist Raoul Pictet (*Étude critique du matérialisme et du spiritualisme par la physique expérimentale*, Genève, 1896) may here be recalled.[1] Professor Naville

[1] The theory of the *potential* of M. Pictet—which Professor Naville occasionally mentions and which ought to be according to its author a starting-point to establish later the existence of moral liberty—is ingenious but worthless. To our knowledge it has found only very scanty approbation in the scholarly world. M. Pictet speaks for instance of the spring, which as long as it remains in a state of tension does not produce all the energy which it could under other circumstances produce. The force is therefore inactive for a certain amount of time; this is "potential" or virtual energy. This means in two words to contradict the law of the constancy of energy. Who does not see, however, the weak point of this argument? The energy is manifesting itself all the time; only instead of doing it all at once within a fraction of a second, it does it slowly and gradually. Keep the spring in the state of tension for a rather long time, it will produce less force when you relax it; keep it in the state of tension long enough, it will no longer produce any energy at all. The example of the stone is not better: a certain amount of energy is required to raise the stone to such and such a height; place the stone on a plank at this height, it will (by its mere weight) manifest only a small amount of energy, by far smaller than the amount that was necessary to take it up; but when the stone falls down, it will give back the virtual energy or "potential." A child could point out the fallacy of the statement. As long as the stone rests on the

is far from being ill disposed towards such attempts. He comes back to the subject several times in the course of the book, and it is even with reference to this point that he expresses one of his most original theses, namely that the spirit of the theoretical sciences, thus of science itself, is fundamentally opposed to determinism (*est foncièrement contraire au déterminisme*). From what has been said above, it is easy to understand how the author reached this point of view: the object of science is a practical one; we would not take the trouble of investigating patiently in order to discover laws of nature, if there was not within us an intimate conviction that we can, by means of them, have an action on the future.

As to us, we feel certain that there is here a deep misunderstanding, and in spite of the fact that the problem has been discussed over and over again, the essential point has not very often been brought to light. It is a mistake to object to the use of the concept of liberty in practical life. It represents with a considerable number of men a powerful spring of action. Only it has nothing to do in science. The strongest proof that it is an intruder there is seen precisely in the tremendous efforts made from antiquity down to our days to find a place for it. There is no more forcible argument against liberty in science than voluminous and classical treatises such as Leibnitz's *Théodicée* or Kant's *Kritik der praktischen Vernunft*. Like so many books written either before or after, they clearly indicate that there is no *natural* place for liberty—and thus contingency—in science: everybody knows it, implicitly recognises it; but since it was thought that in removing this notion from science, it was removing it at the same time from life as a moral factor, considerable pains has been taken to find some excuse in order to keep it in science too.

To quiet those who are afraid, it will be enough to insist upon this: All our sciences, all our knowledge, rest on a conventional agreement. This agreement is determinism. Without determinism no science is possible. The scholar will maintain this in spite of any protest. It is his right to do so,—the condition of his achievements. But if this is granted to him, he will very willingly recognise that this science, resting upon determinism, has no pretention of being absolute or perfect. If you do not embitter him with unfair attacks and quarrels of the theological, ethical, practical, and sentimental fashion, he will go even farther than that; he will tell you himself that determinism is obliged to take up in his system of science contradictory data. And we mean not only the indirect and practical results of theories not thoroughly worked out, but the very principles of knowledge are not consistent among themselves. We explain: Determinism is imposed upon the scholar

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board, gravitation is simply checked, which would otherwise cause the stone to fall. When the pressure of the plane is removed, gravitation manifests itself again. The pretended potential is in no way in the stone, but in the attraction of the earth or whatever the cause of gravitation may be, a force which does not discontinue to act for a moment.—The other cases quoted by M. Pictet could be as easily refuted.

not so much as a result of empirical observation of the world, but first of all as a result of the conditions of perception and of thought. We perceive, we conceive, and we think and reason according to laws: these laws *determine* our knowledge *ab ovo*. The subjectivism of science has been established, as well known, by Kant, but it must be confessed that the actual service rendered by him has not yet been fully realised. These very laws of thought, from which we can escape only in not thinking, impose upon us the famous antinomies of reason. We must conceive of the world as finite, for if we add to each other, or if we imagine that we add to each other, the finite parts of space, we cannot but obtain a finite total. But we must at the same time conceive of the world as infinite; for neither our imagination nor our intellect can think of a limit beyond which there would be no more space. This goes to show plainly that science will never be an adequate knowledge of truth, since truth cannot be contradictory; the universe cannot be at the same time finite and infinite. Only for the systematic conception which we can have of it, it will be necessary to conceive it, conventionally, now as finite, now as infinite.

And conventionalities grow more numerous as we proceed farther on into science. Professor Naville quotes several of them; for instance, the pretended regular geometrical lines traced by the stars. In other cases it is very obvious that they are no longer quoted as conventional, such as the atoms and molecules in physics and chemistry,—nobody has ever seen them; it is probable that they do not exist at all; but their existence is conventionally acknowledged, for it renders investigation easier. Let some one invent another hypothesis which will favor the scholar's researches to a still greater extent, and it will at once take the place of the old tconventionality. Again, who knows anything about the so-called magnetic force, about electricity, affinity? "Words, words, words," the enemies of science may say. Very well; but words thanks to which we are able to understand one another. A meter is something entirely conventional, the meter does not exist in nature: what a powerful help, however, to know nature and act upon it!

On such conventional agreements we succeed in building up a science of the relations of phenomena, a science of determinism. Our first aim in science is to be able to say: that which has been, and that which is, had to be. If now we wish to know the future scientifically also, it will only be in accepting the parallel formula: that which will be, will have to be. Thus we come back to our thesis: There is no special group of canonic sciences; what Professor Naville calls by this name seems to us to be only a series of applications to the future of the scientific data gathered by the *Théorématique*.

We may finally add this: Admit for a moment that there exists really an element of contingency in nature, this would not yet be sufficient to induce the scholar to make a special group of sciences with the *Canonique*. For, this element of contingency or liberty thus introduced into scientific problems would be, by definition, an unknown and unknowable quantity. All that might be granted in this case

would be, that there is a place for a special group of future realities where other than scientific factors will be active, but this domain would then fall outside of the scope of science.

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A STRANGE APPLICATION OF MATHEMATICAL PROBABILITIES.

Poincaré concludes his lectures on the calculus of probabilities[1] at the Faculté des Sciences of Paris with the following skeptic statement: "The calculus of probabilities offers a contradiction in the terms itself which serve to designate it, and, if I would not fear to recall here a word too often repeated, I would say that it teaches us chiefly one thing: i. e., to know that we know nothing."

This opinion of the great French scientist applies in particular to the so-called moral sciences which Condorcet, Poisson, and many other, including even Laplace, attempted to improve by the theory of probabilities. Most of these applications are, as Stuart Mill said, the scandal of mathematics.

In the lines which follow, the writer presents a recent example of this kind, which in his opinion is worse than any of those proposed by Condorcet.

In Professor Lehmann's[2] book on *Aberglauben und Zauberei* (German translation by Dr. Peterson, published by F. Enke, Stuttgart, 1898) on pages 440 and 461, the following statements may be found:

At the first international congress for experimental psychology at Paris, 1889, it was decided to gather statistics and reports on hallucinations. The result of this collection has now been published in two interesting books. One was published by a committee of the Society for Psychical Research, under the direction of Professor Sidgwick, and has the title, "Report on the Census of Hallucinations," Vol. X. of *The Proceedings of the S. P. R.* (date of publication is not given by Dr. Lehmann). The other, *Ueber Trugwahrnehmungen*, was published by Parish (Leipzig, 1894). In the first report, according to Dr. Lehmann, it is stated that "in England and Wales the annual death-rate since the last ten years was 19.15 out of a 1000. If every year 19 die out of a 1000, 19 die every day out of 365,000, or, in other words, 1 out of 19,000. This number $\frac{1}{19000}$ indicates the probability that a person dies on the very day when his body is seen in a hallucination, provided there is no connection between the two events. It may therefore be expected that out of 19,000 hallucinations one will occur during the hour corresponding to the time of death of the person which is due according to the statistics. After the committee had discarded all doubtful cases and taken account of many forgotten

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[1] *Calcul des probabilités*, G. Carré, Paris, 1896.

[2] Director of the Psychophysical Laboratory of the University of Kopenhagen.